SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

TERMS:

Per Year,	Four Dollars
6 Months,	Two "
3 "	One "
Single Copies,	Ten Cents.

Published at

TRIBUNE BUILDING, NEW YORK.

P. O. Box 3838.

London, England, 150 Leadenhall St.

SATURDAY, DECEMBER 31, 1881.

As the present number of this journal concludes the second volume of "Science," the moment appears opportune for us to acknowledge our obligations to the many friends of the journal, who, by their contributions of valuable papers, have assisted in placing it in the distinguished position which it now occupies.

The expression of good will and satisfaction which we have received from so many of our subscribers, is encouraging for the future of the journal, for we must confess that our plans for the editorial management have been but partly developed; if we have deserved such recognition in the past, we look forward for a wide extension of our circulation in the future, when the improvements and additions which we contemplate shall be carried out. Arrangements are in progress to increase the number of pages of "Science" from twelve to sixteen, the four extra pages to be devoted to applied and practical science; in this division the most recent application of scientific principles to the arts and manufactures will find a place, and novel inventions of real scientific merit will be fully described.

Suggestions from our readers respecting any improvement or addition which will increase the efficiency of the journal, will be always welcome, and receive consideration, and we ask our subscribers to introduce "Science" to their friends, and to forward us names of those who in their opinion would desire to receive a sample copy.

We are gratified to find that heads of schools and other representatives of the intelligent classes are sending in their subscriptions, and recognizing "Science" as one of the highest educational journals in this country. The United States Commissioner of Education at Washington has expressed his high approval of the journal in this respect, and we hope to find it in the hands of all men interested in the education of others.

Our subscription list now includes Principals of Schools, Professors in Colleges and Universities, Pastors, Physicians, Manufacturing Houses; and we claim that no person of average intelligence should fail to read "Science," for he will find it a valuable and useful weekly journal, and when bound, a standard work of reference for all time.

An esteemed contemporary calls for a scientific journal, "such as the great body of intelligent people will admit with confidence to their homes." "Science" certainly fills this *role*. The editorial conduct of this journal has been based on a policy to admit the widest discussion of all current scientific subjects, but no editorial bias has been given to any particular set of views. The editor has not himself indulged in polemics, nor permitted the discussion of religious questions, believing that the ground covered by investigations of all branches of the sciences, is sufficient for one journal to cultivate, and that aimless attacks on religious belief are foreign to the purpose of a scientific journal, and inconsistent in a country where religious liberty is guaranteed to all.

At the moment of going to press, a copy of Mr. S. E. Cassino's International Scientists' Directory came to hand. We have, therefore, only time to take a glance at the book, which is a handsome work of 400 pages, containing the names, addresses, special department of study, etc., of those engaged in scientific investigations in America, Europe, Asia, Africa and Oceanica.

About 18,000 names and addresses are given, and the general arrangement of the work is all that can be desired. The Directory will be welcome to all engaged in scientific work, and we trust that it will be strongly patronized, and thus enable Mr. Cassino to continue his good work, which is still far from complete; although so many names are given in this directory, it clearly represents only a fraction of the whole list of scientists, as such well known names as Professor R. O. Doremus and Professor J. W. Draper, are omitted.

We have no doubt that Mr. Cassino has done his best to make his Directory complete, and we congratulate him upon his success, and it simply rests with scientific men to aid him in making future editions perfect.

On receipt of two dollars we will mail a copy of the International Scientists' Directory to any address in the United States or within the postal union.

THE DEVELOPMENT OF HEAT BY MUSCULAR ACTIVITY.

By Professor A. Fick, of Würzburg.

It is the object of physical science in the proper sense of the word, to perceive in all the phenomena of nature the operations of the *same* forces, with which any two material particles always act upon each other, when they come in contact with each other in the same relations. This object has never been so clearly seen by the majority of naturalists, as during the last decade. Since that time a law already proved in mechanics has been recognized as one applicable to all the events of nature. It is called by Helmholtz, who in a treatise which appeared thirty-one years ago, first demonstrated its universal importance, the "law of the preservation of power"; recently the designation "law of the preservation of energy" has also been brought into use by English men of science. The amazing productiveness of this fundamental law of the operation of all natural forces essentially consists in the fact that from it may easily be derived experiments for testing results even in natural phenomena, in which in detail the nature of the acting forces is wholly concealed.

Therefore it could not fail to happen, that since this time individual investigations in the most varied departments of physical science have principally turned upon this fundamental principle. It now seems to me that the results of such individual investigations, which are connected with the most universal points of view, might be best adapted to secure interest even outside the circle of the scientists. In this opinion I will venture to claim the attention of the readers of this publication for some general observations connected with an experiment made by me a short time ago, and elsewhere communicated to persons familiar with such matters.

Each individual can experience in his own body at any moment, that with the aid of his muscles he can conquer opposing forces and set masses in motion. The former happens, for instance, when we lift a burden or throw the whole weight of the person upward in climbing a mountain; the latter occurs when we hurl a stone or swing a hammer. The principle of the preservation of power now demands that, where we see forces conquered or masses moved, necessarily powers on the other side have "acted" or performed labor, that is, that the points of assault of forces have been displaced. This, for instance, is clearly apparent in the voluntary fall of a heavy body. It is the point of attack of a power directed downward, namely weight, and as under the influence of this power it moves downward, its velocity increases; or when in a wavering balance one scale with its burden ascends—its weight is conquered—but the other sinks and its weight performs a certain amount of work. So if by the mediation of muscular action we see forces conquered or masses moved, it must be asked: what powers have acted or performed the labor here, that is, have changed their points of attack in their action.

Forces which, for instance, like weight, act upon larger bodies in a similar manner, will not of course be alluded to here. The point in question can only concern powers that operate even among the smallest particles of muscular substance, that is chemical powers of attraction. Something must take place in the muscle similar to what occurs in the steam-engine, when in the act of combustion under the boiler the particles of carbon and oxygen, obeying their strong reciprocal power of attraction, rush towards each other, makng violent little movements, and a portion of this energy, by means of a series of shocks, is applied to the conquest of opposing forces, or to accelerating the speed of bodies. So in the muscle, during its activity, chemical processes evidently take place, with which powerful kindred forces come into action. That this is really the case can be shown by experiments. Singularly enough, it is not only an analogous, but for the most part at any rate precisely the same chemical power of attraction which performs the work in the active muscle and in the steam-engine, namely the power of attraction between the particles of carbon and the particles of oxygen. The product of the operation of this power of attraction, carbonic acid, appears in a certain quantity at every act of muscular motion.

In all the examples, in which, by the mediation of any arrangements, through whose operation the action of chemical powers of attraction, taking place even in extraordinarily small distances, accelerates the movement of bodies, or overcomes mechanical forces, like weight, a general remark may be made, which has hitherto been everywhere confirmed by experience. The lines of communication between the particles undergoing a change by means of a chemical process are usually irregularly distributed in every part of the space. The movements arising from the individual processes of change are, therefore, also irregularly driven in all directions, and thus can never be applied in their full strength to overcome an opposing force acting in a fixed direction, or to accelerate the speed of a body, whose particles are all moving in the same direction. Only a portion of this collected energy of motion can appear in such a form. A fraction, greater or less, according to circumstances, of the sum of the individual processes of change must retain its original form of the irregularly whirling movement of the tiniest particles. This conclusion may, therefore, be briefly expressed thus: wherever in a chemical process the power of attraction of the smallest particles of different substances performs labor—no matter under what circumstances this may occur—a portion of the labor will always be employed in the development of *heat*.

The *heat* contained in a body is, therefore, nothing else than the energy of slight invisible irregular whirling movements, in which the tiniest particles of the body are included. To increase the temperature of a body, therefore, is merely to increase the energy of these irregular molecular movements of the smallest portions. This view instantly finds support in the common phenomenon, that at the increase of the temperature of a body above a certain degree its particles in consequence of the colossal energy of motion really pulverize each other—"the body evaporates."

If this view of heat is correct, a certain degree of heat can be produced by a certain amount of work. The proportion of work, or the operation of a power is, as is well known, the product of the intensity of the power and the distance through which it has acted. Therefore the product of the unit of the intensity of the power, the *kilogram*, and the unit of the distance, the *meter*, is chosen as the unit of this power. This unit of the value of the work is called the *kilogrammeter*. As the unit of the quantity of heat the same degree has been fixed that is required to be supplied to a kilogram of water, when its temperature is to be raised from 0° to 1° of the Centigrade.

Natural philosophy has now succeeded—and it is one of its most important achievements—in showing, that for the production of a unit of heat an expenditure of work of 425 kilogrammeters is requisite. This number is called the *m chanical equivalent* of *heat*, because it is thereby possible to calculate each quantity of heat in a certain number of mechanical units of work, which is requisite for its production.

The knowledge of the mechanical equivalent of heat enables us to measure exactly the work performed by any chemical process of kindred forces operating even at immeasurably little distances, although we know nothing at all of the laws of action of these forces in detail. In fact, we need only direct the process, so that no effect is produced except the development of heat. If we then measure the heat developed and multiply the number of units found by 425, we shall have the labor which the chemical powers of attraction have performed in the process, expressed in kilogrammeters, since according to the

supposition the whole operation of this labor consisted exclusively in the development of heat. The burning of one kg. of coal may serve as an example; if no other effects are accomplished, about 8000 units of heat will be released. The work which the kindred powers between the atoms in one kg. of coal and the two-fold number of atoms of oxygen accomplish in their union into carbonic acid, thus amounts to 8000 x 425 or 3,400,000 kilogrammeters. From this an idea may be formed of the prodigious intensity of the chemical power of attraction between an atom of carbon and an atom of oxygen. The force with which the particles of carbon, amounting only to one kg., rush from a very little distance to the corresponding particles of oxygen in burning, 'is precisely as great as when a body weighing 3,400,000 kilograms falls from a height of 1 m.

Let us go back with these axioms from natural philosophy in general to muscular action. If, as was shown, there are chemical powers of attraction, whose operation or performance of labor produces mechanical effects which are externally perceptible, besides these, heat must also proceed from every muscular action. This proposition, which we here bring forward as a conclusion from the most universal lessons of the action of powers, has already long been acknowledged as a principle derived from experience.

It was by no means easy to prove this proposition. To be sure, it is rendered extremely probable by the daily experience, that our bodies are perceptibly heated by great muscular exertion, and give off more heat than during the same time with the muscles at rest. But this does not afford an accurate proof. It might be represented that the excessive activity of the muscles only afforded increased opportunity for heat-producing combustion in other constituent parts of the body, for instance in the blood. An exact proof can, therefore, only be given by putting in action a muscle severed from connection with the rest of the body, and proving that heat is developed therein. Such experiments can, of course, only be made on the muscles of cold-blooded animals, because those of the warm-blooded, when separated from the body, lose their vital properties too quickly.

The first person who made such experiments and has shown an increase of temperature, that is a development of heat in *isolated* muscles by action, was *Helmholtz*. This fundamental fact could not fail to attract great attention, and make people endeavor to ascertain what circumstances had an influence on the greater or less development of heat by muscular action. The most important labors in this direction proceeded from *Heidenhain's* laboratory. He has especially much improved the thermo-electric system, which alone can be used to ascertain the increase of temperature of the muscles. With the aid of this system one can distinctly perceive even the extraordinarily slight increase of temperature, which a little frog muscle undergoes at a single, by no means energetic, movement, that scarcely amounts to $\frac{1}{1000}$ of a Centigrade. In successive experiments it can even be determined, in which *more*, and in which *less*, heat was developed, but until now the system has not been thoroughly adapted to fix the absolute value of the increase of temperature.

Some time ago I succeeded in so changing the thermo-electric apparatus, that it is possible, by its means, to fix with some degree of accuracy, the increase of temperature a muscle experiences in its action. Thereby the possibility was instantly afforded, of stating in the usual units the quantity of heat developed by the muscular action. This quantity of heat is namely, evidently, the increase of temperature multiplied by the capacity for heat of the proportion of muscle used, which latter is assumed to be about equal to $\frac{8}{10}$ of the capacity for heat of a body of water of equal size.

According to a general observation previously made, the whole labor performed in the muscular act by chemical powers of attraction can now be definitely determined. For this purpose it is only necessary to allow the muscular action to pass away, so that finally no sort of mechanical effect remains; then, since every labor of forces must leave an effect, a quota of heat will exist that will be the exact equivalent of the work performed by the chemical powers. The condition just expressed may be fulfilled by simply letting the muscle, in its action, raise a weight; but allowing this to fall again, so that it pulls the muscle which meantime has relapsed into a state of rest. In so doing the work performed by the weight of the falling body will evidently be used for the development of heat in the apparatus. To be sure, it might now be asked, in what portions of the whole machinery used, this amount of heat is developed. Theoretically it is beyond doubt, that a portion of it is set free in the intermediate pieces, which connect the weight with the muscle, especially by the friction at the points of union, but since these intermediate pieces are practically non-ductile, and the friction at their points of union can only be very slight, it may be assumed from the beginning, that the quota of heat in question is almost entirely released in the body of the muscle itself, which, by its extreme ductility, receives, so to speak, almost entirely the shock of the falling burden. This supposition is so probable, that in the exact scientific publication of the result of my experiments, I have pre-supposed it as a matter of course. Meantime I have made experiments in my laboratory, which render this supposition one empirically shown.

The experiments have been made in the following manner. A body of known weight fastened to the muscle was raised, not by its own action, but by other labor to a measured height and then allowed to fall. The increase of temperature experienced by the muscle in consequence of the jerk was now measured, and by multiplication with the capacity for heat of the muscle, the quantity of heat developed in the muscle was ascertained. It usually corresponded in a really surprising manner with the thermic equivalent of the mechanical labor, which was applied to raise the appended burden. This affords the proof, that the heat produced by such a jerk is liberated almost entirely in the *muscle*, and only very inconsiderable fractions are developed in the other portions of the machinery used. Every such experiment can thus be looked upon as fixing the mechanical equivalent of heat, which, of course, in point of accuracy, falls far behind the purely physical tests, but is worthy of notice because a living tissue is the means of ascertaining it. To us, however, the interest of these experiments consists in the fact that they prove the reliability of the system used to fix the heat of the muscles.

Let us now return to the development of heat by *active* muscular action, and consider more closely the numerical product of an accurate experiment. That in the estimates of the quantities of heat, and afterwards the value of labor too, many ciphers may not appear immediately behind the comma, we will base them upon units a million times smaller. So, for the unit of heat, we will take the quantity of heat necessary to raise the temperature of 1 mgr. of water from 0° to 1°. As the unit of labor we will choose instead of the kilogrammeter the grammillimeter. The equivalent proportion, therefore, remains unchanged—425. For an experiment a body of muscle weighing 3114 mgr. had lifted in ten pulls, rapidly succeeding each other, a burden of 500 gr. 10 times, and the latter had fallen again as many times, so that at last it hung no higher than at first. The temperature of the mass of muscle was increased 0,0195° by this act. Now, since 3114 mgr. of muscular substance possesses exactly as much capacity for heat as 2803 mgr. of water, the increase of temperature which followed, required $2803 \times 0{,}0195 = 54, 6$ units of heat. But in our experiment the production of this quantity of heat is the *only* effect of the work accomplished by the chemical powers of attraction in the muscular action. It must, therefore, ex-

pressed in the measure of labor, have amounted to 54,6× 425, that is 23205 grammillimeters.

The chemical process, which takes place in muscular action is, it is true, by no means accurately known in the individual stages of its course; but as a whole, it undoubtedly consists in the combustion of a body free from nitrogen, whether fatty or saccharine, to carbonic acid and water. The numbers obtained, therefore, afford us a point, by which to determine what quantities of the above mentioned materials must be consumed in a muscular contraction. We know, through *Frankland's* researches, that in the consumption of 1 mgr. of sugar the chemical powers of attraction perform as much work a is necessary to produce 3800 units of heat. Now, since in the ten contractions of our experiment, 54,6 units of heat were produced, an expenditure of material of 5,46÷3800=0,014 mgr. would have been necessary, under the supposition, that the combustible material was a saccharine body. Let us suppose that the combustible material is a fatty body, then a still smaller expenditure would be sufficient, to produce the effect observed, namely, 54,6÷9000=0,0067 mgr., because 1 mgr. of fat, according to the estimates of the investigator just mentioned, supplied in its combustion, 9000 units of heat. So, for one contraction the combustion of 0,0014 mgr. of sugar, or of 0,00067 mgr. of fat, would have been requisite. If we divide this number by 3.1 (the weight of the quantity of muscle used in grams) the result will show how much material must be consumed at *one* energetic contraction in a gram of muscular substance, that is 0,00045 mgr. of a saccharine, or 0,00022 of a fatty combination. So it appears, that for 1000 energetic contractions not quite 1 mgr. of combustible material in each gram of muscle is requisite, and, therefore, it can no longer surprise us, that only very small quantities of the actual combustible material are ever found in the muscular substance, the greater portion of which, as is well known, really consists of very different materials, principally of substances like the white of an egg.

The results obtained with the new systems can be applied to the decision of the question, what portion of the work performed by chemical powers in the active muscle, can, in the most favorable cases, produce mechanical outward effects. The closest interest in this question might be designated as an "economical" one. In fact, the real object of the animal subject in muscular activity is the production of mechanical effects in the surrounding universe, and one might denote the portion of the work accomplished by chemical powers, which is applied to the mere production of heat, as an inevitable loss from the point of view of animal economy. At any rate, one will have the more reason to admire the judicious arrangement of the muscular substance, which can apply a larger portion of the chemical labor performed in it to external mechanical results.

It is precisely the same as in the steam-engine, whose construction we also call the more perfect, according to the larger portion of the work performed by the chemical powers of attraction in the burning of the coal it allows to be used to produce mechanical effects. In spite of the most eager efforts of technics hitherto no attempt has been successful in making more than $\frac{1}{10}$ of this labor mechanically effectual. Fully $\frac{9}{10}$ are lost to the objects of the machine, by being inevitably employed in the production of heat, which at the utmost can only be used for minor purposes, such as the heating of rooms and similar objects.

If it must now be ascertained, how the muscle is situated in this respect, it is only necessary to fix, by experiment like the one above described, what mechanical effect has been accomplished in a given time, and compare this measured in the proportion of work, with the chemical labor calculated by the heat produced. It will be advisable to pay special attention to the fact, that the heat finally developed would be less by a corresponding amount, if the experiment had been so arranged, that the mechanical effect, that is the raising of the weight, had been maintained. The quantity of heat corresponding with this effect was first released in the muscle by the falling of the burden again.

By the 10 contractions of the foregoing experiment 500 gr. were raised on an average about 1.3 mm. high. Thus the mechanical result amounted in the whole to 6,670 grammillimeters. The work performed by chemical powers of attraction in the 10 contractions we have found above—23,205 grammillimeters. This number is about $3\frac{1}{2}$ times 6,670. Thus, by these contractions, somewhat over $\frac{1}{4}$ of the whole chemical labor was applied externally and not quite $\frac{3}{4}$ to the direct production of heat. That in the actual experiment this quarter was also finally converted into heat, depended merely on the external arrangements, which permitted the burden raised to fall again each time.

We see by this, that—as was to be expected—the muscle machine is very superior to even the most perfect steam-engine, in so far that it *can* employ the combustible material twice as frugally for the same main object.

Besides, this relation between mechanical action and development of heat is by no means obtained at *every* muscular contraction. I have intentionally selected from my experiments as an example, the one in which the mechanical labor amounts to the largest fraction of the whole chemical labor. To obtain this most favorable proportion, the burden must stand in a certain relation to the thickness of the muscle. If the burden is larger or smaller, a smaller portion of the chemical work will be used for mechanical act on, or—as it might be expressed—the combustible material will be less economically used. This proposition may be demonstrated *a priori*, for it is easily seen, that in the two extreme cases, where the burden is a cypher or infinitely great, chemical work is performed and heat developed, but no external mechanical action is obtained.

The solution of the question, in what relation the mechanical action for the development of heat can stand, under the most favorable circumstances, towards the muscular contraction, enables an observation to be made which throws new light upon the change of substance in animal bodies. As is well known, the change of substance in animal forms may be designated in general as a process of combustion. In reality, a certain quantity of combustible nutritious matter daily enters into the fluids, and a corresponding quantity of oxygen is taken in with the breath. On the other hand, every day on an average, a precisely similar quantity of substances is withdrawn, whose combination is to be regarded as the product of an almost total combustion of the nutritious matter. The condition of the body with this equal balance between receipts and expenditures, remains for a long time apparently unchanged.

With the formation of the product of combustion from the assimilated nutritious matter and the inhaled oxygen, the colossal power of attraction of this element for the elements of the nutritious matter, especially for the carbon and hydrogen gas, now performs a fixed amount of labor, which is independent of where the combustion takes place, and whether it occurs at once or in various stages at various places.

People were formerly inclined to suppose, that the greater portion of the combustion in question occurs either in the fluids themselves or in special organs, such as the liver, the kidneys, etc.

Since the changes occurring in animal bodies have begun to be viewed from the standpoint of the principle of the preservation of power, it must be looked upon as a self-evident truth, that at least a certain portion of the assimilated nutritious matter passes into the muscles, to be first consumed here, since from the point of view of that principle, the mechanical performance of the muscles can only be understood as the action of the labor of the

chemical powers of attraction, as we have done in the preceding discussions. The question may now be raised, how large a fraction of the whole combustion takes place in the muscles, and how large a fraction in the other parts of the body? The distribution of the process of combustion in the different places might, therefore, be accomplished in two ways. One part of the material might be consumed entirely in the muscles, the other entirely elsewhere, or certain stages of the combustion of the whole material might take place in the muscles, and other stages in other places. However this may be, as it is supposed that a considerable portion of the combustion takes place outside of the muscular substance, it must be expected that, under all the circumstances, far more than $\frac{3}{4}$ of the whole heat of the combustion of the assimilated nutritious matter in animal bodies appears as heat, and only the equivalent of far less than $\frac{1}{4}$ is available for mechanical labor. For, as we saw, even under the *most favorable* circumstances, about $\frac{3}{4}$ of the chemical work performed in the muscle itself is inevitably used for the production of heat. But under these most favorable circumstances, however, probably *all* the muscles do not take part in the labor of the whole body. Therefore, in the acts of living beings we must assume that more than $\frac{3}{4}$, probably $\frac{4}{5}$ of the result of the work performed in the muscles by chemical powers, finally appears as heat. Now, if the material coming into the muscles for combustion should be even a moderate portion, for instance $\frac{1}{3}$ of the whole assimilated nutritious matter, while $\frac{2}{3}$ was consumed elsewhere, then $\frac{14}{15}$ of the chemical work performed by the whole combustion is used for the mere production of heat, since $\frac{2}{3}$ of the labor accomplished outside of the muscles can have only the result of producing heat, and of the third coming from the muscle, $\frac{4}{5}$ will also produce mere heat. So, under this supposition, it must be expected, that at the utmost the equivalent of $\frac{1}{15}$ of the heat proceeding from the combustion of the nutritious matter would be available for the mechanical effects of the organism, externally.

It is already more than twenty years since Hemholtz, by very convincing arguments, proved from facts known at that time, that in seasons of extreme muscular labor, for instance, climbing a mountain, the measureable mechanical performances of the whole organism are proportionally considerably greater. They are equal to the equivalent of about $\frac{1}{5}$ of the heat of the consumption of the material that burns during the time of these performances, in the whole body. Unless the supposition is now made, that the muscles of mammalia can work incomparably more economically than the muscles of the frog—a supposition wholly unjustified by our knowledge of the properties of the muscular substance in the different bodies of animals—we must conclude that, in times of extreme muscular activity, the whole process of combustion takes place in the muscles and the chemical processes going on in other parts can only be those in which the chemical powers of attraction accomplish no considerable labor. In fact, from the results of our experiments concerning the heat of the muscles we have inferred, that by the chemical labor performed in the active muscles themselves in the movements of living beings, fully $\frac{4}{5}$ is employed in the production of heat; but if chemical labor was performed in other parts of the body, whose whole result could be only a purely thermal one, more than $\frac{4}{5}$ of the chemical labor performed in the whole body must go to the production of heat, and less than $\frac{1}{5}$ would remain for mechanical external actions.

If it is once proved, that in times of extreme muscular action the processes, by which the chemical powers of attraction perform labor, take place almost exclusively in the muscles, a similar performance will occur even in times of comparative muscular rest; for otherwise it must be supposed, that the change of substance during the period of rest takes a totally different direction from that during the time of muscular activity, which is scarcely conceivable. Yet it must be supposed, that in animal bodies a certain kind of combustible material is prepared for the machinery of the muscles, for which in other portions the conditions of combustion do not exist, as coke cannot be burned in a stove arranged for wood. We shall, therefore, be compelled to suppose, that the process of combustion, which renders muscular labor possible, glimmers continually in this texture even in times of rest, only with so little strength, that there is no mechanical action, and only heat is produced.

There is a very note-worthy harmony between this inference and an assertion made by Pflüger and some of his pupils on the basis of very different facts, which is, that in the muscles, even during periods of rest, processes of combustion occur, which are under the influence of the nervous system; they can be kindled to considerably higher degrees of intensity, before attaining the point requisite for the purpose of a visible mechanical action of the muscle. The increase in the department of these lower degrees of power would, therefore, lead only to an increase of the production of heat, and according to the well-founded hypothesis in question ought to explain the fact, that the development of heat in animal bodies can exist under conditions of the loss of heat externally.

From all this one would form the following idea of the course of the chemical processes, by which the assimilated nutritious matter is transformed into the rejected matter. The nutritious matter enters into the blood, the liver, and other places only during the chemical processes in which the chemical powers of attraction either perform no considerable work, or in which as many chemical powers of attraction are conquered as come into positive action. These may be partly synthetic performances, partly disunions. Above all, it must be supposed that the greater portion of the nutritious albumen undergoes, directly after its reception into the fluids, a process of this nature, in which a body containing nitrogen is separated, that soon leaves the body under the form of urine. The remnant of the nutritious albumen, free from nitrogen and the other nutritious matter rich in carbon and hydrogen, is then supplied to the muscles as combustible material, perhaps loosely united with the oxygen received by the breath. In action, however, the vast powers of attraction between the atoms of oxygen on one side and the atoms of carbon and hydrogen on the other, first enter into the muscular tissue, whereby in the formation of carbonic acid and water, partly heat and partly mechanical effects proceed.

I should consider the object of these lines attained if I succeeded in showing how a few insignificant thermometrical experiments in frogs' muscles are capable, from the point of sight of the principle of the preservation of power, of casting a new light on all the particulars of the nourishment of the human body.—*Translated from "Deutsche Rundschau," by M. J. S.*

DR. T. S. COBBOLD exhibited (at the Linnean Society's meeting, November 5) under the microscope about a hundred eggs of *Bilharzia hæmatobia*. They were taken from a gentleman who had just arrived from Egypt, and who was the victim of hæmaturia, supposed to have been contracted during a shooting expedition. By adding water nearly all the eggs were hatched during the meeting of the society, and a rare opportunity was thus afforded of witnessing the behavior of the newly-born ciliated animalcules.

DONATION TO AID SCIENCE. Mr. Charles Crocker has made the very handsome donation of $20,000 to the California Academy of Sciences, the income of which is to be devoted to aid worthy and studious investigators in any branch of science, who, by their scientific work, have excluded themselves from acquiring support through the ordinary avocations of current industrial life.

M. PAUL BERT, the new French Minister for Public Instruction, is said to be a candidate, in the section of Medicine, to fill the place vacated in the Academy of Sciences by the recent death of Dr. Bouillaud.

EXPERIMENTS OF M. BJERKNES.

"INVERSE" IMITATION OF ELECTRIC AND MAGNETIC BY HYDRO-DYNAMIC PHENOMENA.

It must be confessed that absolutely nothing is known of the real nature of electricity. The principle of the conservation of energy and that of the unity of physical forces, which tends to simplify the phenomena and embrace them under one common term, for want of a clear and precise definition lead us to consider electricity or rather electric phenomena, as a *mode of movement*. This conception removes the difficulty, simplifies the question, but does not resolve it. The mode of movement, once admitted, forcibly compels the abandonment of the idea of *fluid*, which always accompanied electric phenomena at the outset of their study.

If we are agreed to-day upon the immateriality of electricity, we are, on the other hand, far from understanding the nature of the special mode of movement which characterises electricity, the word being taken in its most general acceptation.

In his *Recherches sur l'électricité*, M. Gaston Planté expresses his ideas on this point in the following words: "Electricity can be regarded as a movement of *ponderable matter*—movement of *transportation* of a very small mass of matter incited with very great velocity, if the question is of the electric discharge, and a very rapid *vibratory* movement of the molecules of the matter, if the question is of its transmission to a distance under the dynamical form, or of its manifestation under the statical form at the surface of the body."

For some, who are much less precise in their definitions, electricity is produced by *molecular movements*, without otherwise determining its nature, characterised by form, direction, velocity, periodicity, &c.

In the experiments about to be described, M. Bjerknes proposed to throw light upon the question of the nature of molecular movements, by reproducing *mechanically*, but INVERSELY, simple and fundamental electric phenomena

M. Bjerknes designates under the general name of *vibration*, the movements which take, according to their nature, the name of *pulsation, oscillation*, etc.

Pulsation has reference to the change in volume. It includes two phases, one, in which the body swells, the other in which it shrinks. Pulsations are *synchronous* when the phases commence simultaneously.

Oscillation has reference to the change in place, it is an alternative displacement to the right and to the left.

M. Bjerknes mechanically obtains pulsations in water by the aid of a very ingenious apparatus.

The *pulsations* are produced by small cylinders stopped at their ends by flexible walls. A small hand pump which is partly shown on the right in fig. 1, is employed to exhaust and compress alternatively the air in the cylinders provided with flexible walls, at a great velocity.

In the simplest pulsator, the two walls dilate and contract at the same time under the action of forcing the air from the pumps, the phrases are synchronous (fig. 2. No. 1). In another arrangement, the two drums are separated by a rigid wall, which forms two chambers each in connection with a separate pipe conducting the air (fig. 2, No. 2). We have thus a most complete system, for by adjusting conveniently the tubes of the air-pump for exhausting and filling, synchronous pulsation can be produced at will, as in the first case, or pulsation in which the phases are alternate.

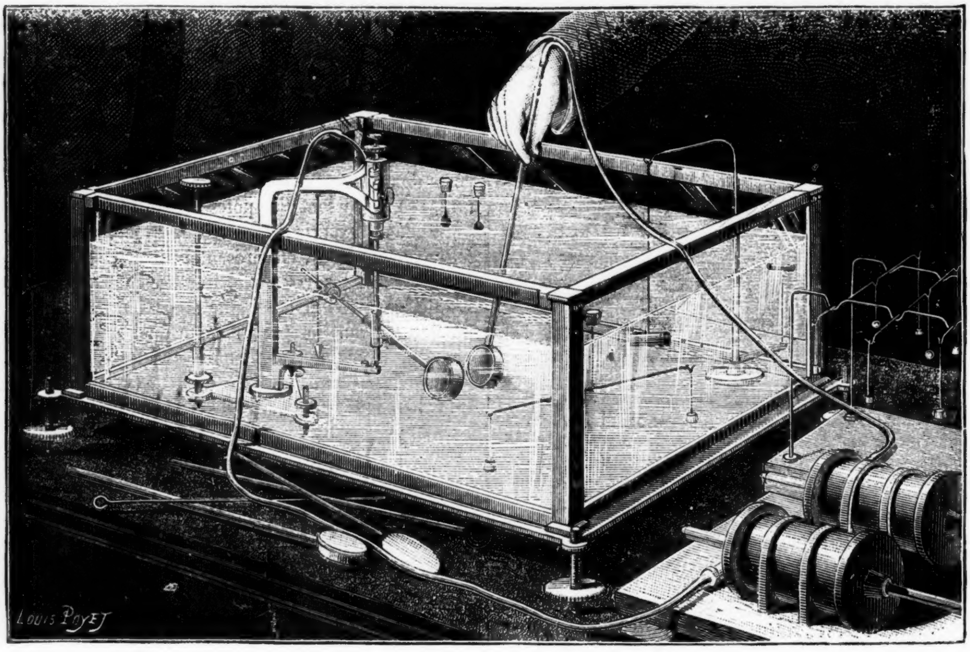

Fig. 1. Apparatus of M. Bjerknes at the Electrical Exposition.

Oscillations are produced by means of small metallic spheres bound to supports, upon which they oscillate, under the action of compressed air, in a plane which varies with the position of the sphere.

Fig. 2 (No. 3) represents two of these oscillators, the sphere on the right oscillating vertically up and down; that on the left, on the other hand, oscillates horizontally from right to left.

This is the very simple and well constructed apparatus which M. Bjerknes employs. Now we come to the phenomena.

First two pulsators are taken and made to vibrate. The phase of dilation, according to M. Bjerknes, correspo ds

to the north pole, the phase of compressing to the south pole. Now bring to one of these pulsators which can turn freely around a vertical axis which acts as its support, while allowing the vibration to continue, a second pulsator held in the hand.

If we put in juxtaposition, in the liquid, the two pulsators whose phases are of the same kind, synchronous, poles of the same name will always be in juxtaposition, there will be *attraction*, the movable pulsator turning on its axis, will tend to approach the pulsator held in the hand by the experimenter, and it will follow it if it is moved. If the phases are changed, so that they are inverse, opposite poles will be together, and there will be *repulsion*. In the one case as in the other, the attractive or repulsive force is proportional to the intensity of the pulsations and inversely proportional to the square of the distances. In both cases, the hydro-dynamic effect is the reverse of the magnetic effect: similar phases attract (poles of the same name repel each other), different phases repel (opposite poles attract).

The same experiment is repeated with the oscillators (Fig, 2, No. 3); by presenting to a vibrating sphere, movable on an axis, a second vibrating sphere, attraction or repulsion is produced according to the synchronism or the discordance of vibration, which the parts of the sphere in juxtaposition present at each instant.

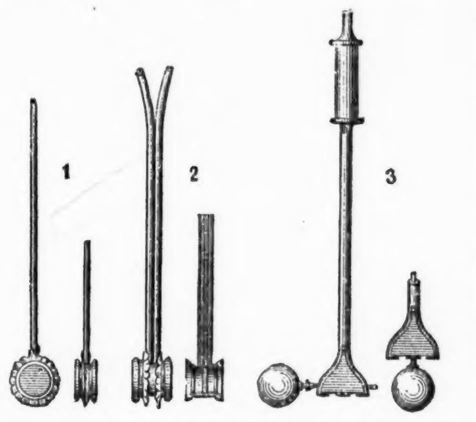

Fig. 2. Apparatus for reproducing pulsations and oscillations in a liquid.
1. Simple drum. 2. Double drum. 3. Sphe es.

For these phenomena, the arrangements for which can be varied, M. Bjerknes has a collection of apparatus almost complete, representing inverse analogies to the phenomena of the reciprocal action of two permanent magnets. A result similar to the action of a magnet on a piece of soft iron can be obtained. By presenting, in the water, a small metallic sphere to a pulsator, or to an oscillator, the small sphere will be attracted.

The effects of *diamagnetism* are shown by means of a small sphere lighter than the water, maintained at the middle of the liquid by a thread attached to a weight which ballasts it. By bringing a pulsator or an oscillator near this sphere, the latter will be repulsed.

From these experiments, and from others the details of which cannot be given, M. Bjerknes concludes that the motion in water of a vibrator (pulsator or oscillator) produces in this fluid a real magnetic field with its lines of force, presenting, *but always inversely*, phenomena similar to those of diamagnetism, paramagnetism, magnetic interference, etc.

M. Bjerknes has even succeeded in tracing the directions of the lines of force produced in the liquid, by means of the arrangement shown in fig. 3. For this a light bowl sustained by an elastic rod is placed in the middle of the liquid; this bowl having no motion of its own will take exactly the direction of the oscillation of the ambient medium. If it is surmounted by a small brush, the latter will paint faithfully and automatically on a sheet of glass the lines of force of the field under the influence of which it oscillates.

M. Bjerknes commenced by submitting all these questions to analysis, and the results of his experiments are only the rigorous confirmation of his calculations. In that which concerns the analogy between the electric currents and hydrodynamic action, M. Bjerknes recognized that the question is not as advanced as in magnetic phenomena.

In order to produce more complex movements, the vibrators are no longer suitable. M. Bjerknes attempted to realize them in a viscous liquid, and striking analogies were found between the lines produced by hydro-dynamic phenomena under these conditions and the lines obtained by real currents under corresponding conditions, but the results obtained are not accurate enough to enable one to form an opinion.

What now can be concluded from the experiments of M. Bjerknes? The fact indisputably established is as follows:

Mechanical vibrations produced in a liquid medium cause phenomena analogous, but *inverse*, to the magnetic phenomena produced by magnets. From this can be concluded, by *analogy*, but not *absolutely*, that molecular vibrations of a different nature can produce *direct* phenomena.

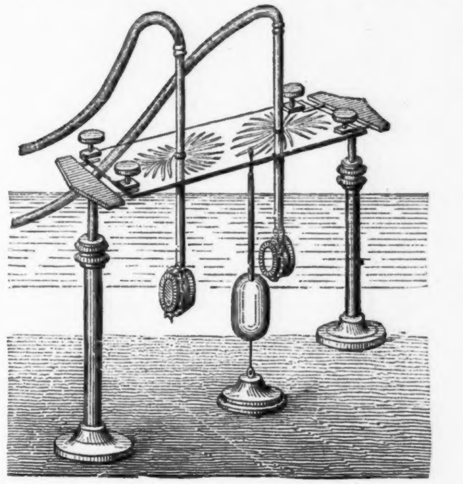

Fig. 3. Apparatus of M. Bjerknes for tracing automatically the lines of hydro-dynamic force.

If this is not a new *proof*, in the exact meaning of the word, of the *vibratory nature* of magnetic and electric effects, it is at least a powerful argument applied to this view, accepted at the present time by most physicists.—*Translated from La Nature.*

M. PLATEAU describes as "*un petit amusement*" the following experiment:—a flower like a lily, with six petals each about an inch long, was constructed in outline in thin iron wire, the wire being first slightly peroxidised by dipping for an instant into nitric acid. This wire frame was then dipped into a glyceric-soap-solution, which, when it was withdrawn, left soap-films over the petals. The stalk was then set upright in a support, and it was covered by a bell-glass to protect it from air-currents. In a few moments the most beautiful colors made their appearance. If the solution is in good condition the films will last for hours, giving a perpetual play of color over the flower.

SHALER AND DAVIS' "GLACIERS."

By W. J. McGee.

(*Continued from page* 584).

VII. *Ancient glacial periods.*—Since the records of glacial phenomena are mainly such as are likely to be obliterated by succeeding geological mutations, it is needless to look for as unequivocal testimony of glacial periods as that attesting the occurrence of the last ice age. The principal evidences we can hope to obtain are (1) tide-washed boulders, gravel, and sand, (2) erratics dropped from icebergs in the deeper sea deposits, and (3) paucity or absence of organic remains, or possibly fossil forms suggesting low temperature; and it is to be expected that such evidence will become constantly less explicit as the geological record is traced backward.

Anterior to the Quaternary ice period, we first come upon evidence of a Miocene glacier, well marked in the hill of Superga, near Turin; from which evidence it may be inferred that the European continent underwent a very severe glaciation in Miocene times. Next follows the Eocene, in which the Flysch of Eastern Switzerland, formed of conglomerates containing immense boulders, supposed to be derived from worn-down mountains of the Vosges or Black Forest, eloquently attests vigorous ice-work; while "in North America we can almost mark the line of the ice by the limit of the destruction of the Tertiary beds" (p 95), and are hence without so characteristic ice deposits. The Cretaceous affords no evidence of glacial conditions save occasional iceberg-dropped boulders; but in the Jurassic are found the conglomerates of Northern Scotland and of the Connecticut Valley, which appear to be of glacial origin. The traces of Permian glaciers are unmistakable, and of almost world-wide extent, being found in Central England, in Scotland, in the Isle of Arron, in Ireland, in South Africa, and elsewhere. The rocks of Carboniferous age include conglomerates, probably of glacial origin, from Southern France to Scotland, from Alabama to New Brunswick, in India, and in other countries; from which it appears "that it extended ice action over a wider meridional range than any [periods] that have succeeded it" (p. 98). There are then no beds certainly attesting ice-work until the base of the Cambrian is reached, when the extensive Ocoee conglomerates and Chilhowee sandstones, and lesser deposits of like character at Roxbury, Mass., and elsewhere, are found. In many of the foregoing cases the associated beds indicate warm or subtropical climate, as should happen according to Croll's theory.

VIII. *The climatal conditions of the glacial periods.*—There is no reason to believe that ice periods were ever of particularly low temperature. On the contrary, the Quaternary fauna was characterized by the great size of its individuals, and clearly proves the contemporaneity of a luxuriant flora, such as could not exist in arctic cold. Moreover, there are abundant proofs, that this glacial period was a time of much greater rainfall than the present. Accordingly several glacial hypotheses may be summarily dismissed. That of Poisson is qute untenable. It is questionable whether that attributing climatal oscillation to variation in the constitution of the atmosphere should be admitted to have weight; though effects resulting from such a cause would be cumulative. Croll's theory is found to harmonize strikingly with the observed facts; There is the last period of high eccentricity occurring at the proper date for the Quaternary ice age; there are the numerous successive inter-glacial periods corresponding with the alternate advances and retreats of the ice; and there are the brief epochs of warm climate, represented by luxuriant floras and vigorous faunas. On the other hand, however, there are the objections that some minor factors may possibly have been overlooked in framing the theory; that there are vast ages without evidence of glacial action, as should not occur, according to the theory, since "the eccentricity of the earth's orbit is such a constantly recurring phenomena" (p. 107); that the Antarctic glaciers are not advancing, but that all observations "lead us to the conclusion that the ice there is as much in process of retreat as it is in the Northern Hemisphere" (p. 107); and that, the hypothesis assumes essentially the same outline for Cape St. Roque during the glacial periods as at present. But none of these objections are fatal to the theory; the only question is as to its being a sufficient cause of anything so wide-spread as the continental ice periods. There are also elements of probability in the hypothesis of Le Coq, that variations in solar emissivity might produce glaciation; for augmented temperature would increase precipitation and lead "to an extension of the fog envelope which in all glaciated regions does so much to protect the ice from the sun" (p. 108); but on the whole, though this hypothesis has the advantage of ndefiniteness, it is entitled to less weight than that of Croll. Neither must the hypothesis of minor geographical alterations be overlooked, since it is perhaps possible that such changes may have occurred in such manner as to facilitate glaciation. The question, however, remains an open one, "and it is unsafe for the geologist to commit himself definitely to any of the hypotheses that have been suggested" (p. 110). There are half a dozen distinct and powerful causes, together with a number of minor factors, which co-operate to produce the singular uniformity of terrestrial temperature; and the only safe conclusion is that the earth's secular winters may be due partly or wholly to any or all these agencies.

IX. *Effect of glaciers on the altitude of the lands.*—Throughout northern regions there are evidences of considerable depression of the land during the glacial period; which depression in Europe was variable, and mainly confined to the severely glaciated area. "In America this depression has not been studied except along the Atlantic shore" (p. 113), where it increases from twenty feet near the southern limit of glaciation, to over two thousand feet in Greenland. The land appears to have remained below the normal level until after the withdrawal of the ice. Two hypotheses have been framed to account for this depression:—1st. That of Adhemer, which attributes the phenomenon to the dislocation of the earth's centre of gravity by a polar ice-cap, and which is based on the assumption of a rigid terrestrial crust. The depressions at various latitudes were not, however, of the relative value demanded by this hypothesis. 2nd. That of local deformation of a flexible terrestrial crust beneath the weight of the ice. This hypothesis is supported by the greater part of the evidence thus far collected; though it is likely that both classes of agencies co-operated in producing the effect. There is reason to believe, also, that a temporary upheaval of lands south of the ice-sheet occurred during the Quaternary; which upheaval was doubtless a concomitant of the local depression beneath the ice.

X. *The effect of glaciation on the life of the earth.*—During the growth of the ice-sheets there must have been a widely extending southerly migration of animals and plants, giving rise to individual and specific variation in consequence, not only of the change of habitat, but also of the crowding of individuals over the contracted habitable area; and the converse movements following the withdrawal of the ice must have been nearly as important biologically. The development and extinction of the hairy mammoth affords an illustration of the effects of secular winter on animal life. "In the closing stages of the glacial period we find him the most widely disseminated of all the large mammals that are known to us" (p. 119); his remains occurring alike over Europe, Asia, and America. "As we go back into the glacial time, we have fewer and fewer indications of the existence of this noble beast, yet we have remains enough to make out that he or his immediate ancestors existed at the beginning of that epoch, and that in all its stages he was feeding

in the rich forests that seemed to have flourished close to the walls of ice" (p. 119). In Europe, and perhaps in Asia and America he was a contemporary of man. In Asia, he dwelt in vast numbers on the plains of Siberia. "When he abounded there, the climate was * * * as cold as it is at present. The rivers in that country have their sources farther to the south than their main streams, so that the springtime sends down a torrrent of water before the more northern channels are released from their wintry bonds; the elephants seem to have herded together along these streams for winter quarters, * * * and to have been swept away to the north by the inundations. These freshets carried their bodies to latitudes where the cold was so great that they were frozen in the mud that wrapped them round and covered them to such a depth that the brief summer-times never melted their icy casing" (pp. 119-20). Some of these bodies remained undecomposed up to the time of their discovery in recent years; and their tusks yet occur in such numbers as to be of commercial importance. The low temperature under which the mammoth existed is attested not only by his hairy covering, but by the coniferous vegetation upon which he, with his congeners, appears to have subsisted. Whether his final extinction was accomplished by human agency is a question; but it is little less than certain that he entered the glacial era with man, was hunted in Europe, Asia, and America by paleolithic savages, and survived until the amelioration of the glacial climate.

XI. *Relation of glaciation to the history of man.*—The evidence concerning man's relation to the glacial period is divisible into two categories:—(1) that which connects him with the closing stages of that epoch; (2) that which establishes his existence previous to the advent of the ice. The evidence belonging to the first category is overwhelming in quantity; it mainly consists in the finding of human bones and products of art associated with the remains of glacial animals or imbedded in later glacial deposits. That belonging to the second class is much more meagre, and has been obtained satisfactorily in only three localities, viz.; central France, California, and New Jersey. In the first of these localities a human cranium was discovered in volcanic tufa beneath a sheet of lava, associated with a fauna whose general facies is ancient, though not sufficiently definite to establish the pre-glacial existence of man paleontologically. The pre-glacial age of all these remains may be, however, inferred from the evidence furnished by the sub-ærial erosion of the valley of Le Puy and the glacial erosion of the adjacent mountains of Coutal; though the testimony can hardly be regarded as conclusive. The finding of a human cranium in auriferous gravels overlain by extensive lava-beds probably of pre-glacial age in California, associated with organic remains of rather more southern type than those of Le Puy, as attested by Whitney, affords more satisfactory evidence of the pre-glacial existence of man. Along the Delaware river in New Jersey numerous rough-stone implements have been collected by Abbott from a table drift, or mass of re-arranged glacial matter, which is destitute of organic remains. "From a rather incomplete study of the ground, the only view I could take of these remains was that they were scattered on the surface of the earth to the northward before the last glacial period; that they were thrust before the glacier during its period of greatest extension, and deposited in the beds where they now lie by the action of water, while the above underwent a slight submergence" (p. 134). These chipped flints of the Delaware, no less than the Le Puy and Calaveras eronia, indicate that even at this early day man had attained a social condition similar to that of the European stone age; and hence that during the vast intervening period, the duration of which was probably not less than 200,000 years, or forty times the term of recorded history, there was almost no progress in the latterly rapid process of intellectual development.

XII. *The movement of glaciers.*—"It is to DE SAUSSURE that we owe the first hypothesis concerning glacial motion" (p. 140); his view being embraced in the suggestion that the ice slid bodily downward in a solid mass, the sliding being facilitated by melting of the basal portion of the ice through the influence of proper terrestrial heat. The utter inadequacy of this hypothesis must, however, have been apparent to its talented author;—indeed—"we are forced to believe that this statement does not represent his conclusions" (p. 140). Charpeutier subsequently suggested that the motion is due to the nightly freezing and expansion of the water taken into the interstices of the ice during the day; but this hypothesis is also inadequate. Still later the solution of the problem was undertaken by AGASSIZ, who devised a critical series of experiments to determine the empirical laws of motion of the glaciers. The plan was to plant a line of stakes across the ice-stream, and to measure their absolute and relative movement at the end of a year. During the first season the entire series was overthrown by the superficial melting of the ice; but the stakes were again more firmly planted. Among the naturalists who visited his camp on the glacier was J. D. FORBES, whose contributions to glacial physics are well known. "While the guest of Agassiz, Professor Forbes made his first acquaintance with existing glaciers. Owing to his superior training in the branches of learning that this peculiar problem called for, he soon saw that the method that Agassiz was using was, by a slight modification, capable of a more speedy solution than his Swiss host could obtain under the conditions of his experiment. Agassiz planted a row of stakes across the glacier, but proposed to wait, with the patience that characterized his mind, until after a winter, to read the answer he sought. Mr. Forbes saw that with a transit or theodolite he could, in a few days at most, see how the stakes were moved, and so anticipate the results his host was seeking. With this plan in mind he went to the Mer de Glace, set up a line of stakes in the precise position devised by Agassiz, and within a month proved that the ice moves most rapidly in its middle parts, and not, as had been supposed, more quickly upon the sides of the stream; this result he hastened to make public" (p. 142). These, as well as later observations, show the laws of motion of glaciers correspond to those of moving liquids. Somewhat previously RENDA had reached a similar conclusion. FORBES soon after enunciated the viscous theory of ice-motion, illustrating "his conception of glacial movement by frequent reference to other substances, the the viscosity of which we recognize in ordinary experience, such as tar, wax, or molasses" (pp. 143-4). "In the hands of his followers this theory has sometimes assumed a different shape * * *; it is then made to mean that the ultimate tangible elements of the glacier, the bits of ice into which it is divided, slide over each other, as, for instance a heap of peas when poured on a sloping surface" (p. 144). This captivating hypothesis has not, however, been widely adopted. Next followed the fracture-and-vegetation hypothesis of Tyndall; but neither has this view commanded general assent. Still later Croll enunciated the hypothesis of successive melting and freezing of the molecules of the ice; "each molecule, as it is melted, parting with its heat to its neighbor on the inward side of the ice, and returning to the solid state, shortly to be remelted by the heat transmitted by its outer neighbor" (p. 145). There are certain phenomena, however, which this inherently probable hypothesis fails to explain. There is then the view in which the motion is referred to momentary melting, through the influence of pressure, of the particles of ice from time to time subjected to unusual strain; but, like the last, this hypothesis is alone incompetent to explain all the phenomena of ice-motion. Finally there is the sliding theory of Hopkins, which is in conflict with all we know of glacial movement. Examples of glacier motion are furnished by the annual snows of New England hill-sides; and the energy of this motion is well illustrated by a phenomenon observed

on a terrace in a cemetery at Augusta, Me. "On this terrace snow accumulated one winter so as to fill up the re-entrant angle it formed with the hill-side. When in the spring this snow melted away, it was found that the upright tombstones and the iron fence that surrounded the graves were broken off near the surface of the ground, and moved in the direction of the general slope of the hill" (p. 148). Summing up the foregoing hypotheses in their application to glaciers of the alpine type, and viewing them in the light of the various phenomena recorded, it appears that all except the sliding and viscous theories are "true causes" of ice-motion; and though neither is alone competent to explain the various phases of the movement, all must be recognized in a satisfactory and consistent theory. As in the theory framed to account for glacial periods, so in this case also, our theory is sufficiently flexible and indefinite to be sure of coinciding with the truth in some of its aspects.

"The problem with which we have to deal when we come to the task of explaining continental glaciers is of quite a different nature" (p. 151), since in this case "we have to leave gravity, as it works in Swiss glaciers, almost out of account" (p. 154). The ice might move freely for a score of miles from its southern border; but in the interior little if any motion would probably occur, except such as might result from pressure-melting; the excess of water formed in this manner escaping in sub-glacial streams. "In this way we may conceive that the ice of British America may have been carried out, from centre to periphery, in the form of water, and the waste of its grinding borne along by the streams that were formed by the pressure-melted water" (p. 159). In other words, continental glaciers appear not to move as ice, but only as water, except along their extreme peripheries.

XIII. *Certain effects of glaciers.*—Outside of glaciated regions the soils and sub-soils are the products of simple weathering conjoined with vegetal action, and may be denominated *soils of immediate derivation*: while within such regions the superficial accumulations are made up of mechanically comminuted materials brought thither from numberless localities, some perhaps hundreds of miles distant, and may be termed *soils of remote derivation.* Since the soils of the first class vary with the character of the underlying rocks, it follows that the latter are more uniform in constitution over considerable areas; and they are at the same time more durable, for not only are the materials essentially identical in all parts of the thickness of the deposit, but they also contain desirable mineral constituents locked up in the included pebbles to be gradually liberated by atmospheric and chemico-vegetal action. The glacial clays were "laid down in a very unoxidized state. Generally they are of a bluish hue, and only attain the ordinary yellowish or reddish color of decomposed clays as the waters acidulated by vegetation slowly penetrate into them." "In North America this penetration of atmospheric decay is distinctly proportionate to the nearness of the clays to the old glacial front" (p. 165); and hence affords a rough measure of the length of post-glacial time.

The coarser moraine debris constitutes, however, but a small part of the glacial waste;—the impalpable glacial mud must have been formed in scores if not hundreds of times its volume, and swept for the most part into the sea to build up azoic shales and clay slates, perhaps intercolated with conglomerates, as in the Roxbury conglomerate series near Boston. This fine glacial detritus, whether accumulated in river-beds, lake-bottoms, or unfossiliferous marine formations of any age, may be distinguished from river salt by its unoxidized state and blue color. The distribution of both the coarser and the finer glacial products has been largely accomplished by marine forces, after they were thrust by the ice into the sea; as in the case of the Tertiary sands of the southern states, which were probably originally brought to the ocean by glaciation along the more northerly Atlantic coast.

The formation of auriferous gravels and the general accumulation of gold in drift by glacial action, was accomplished by the simple concentration of heavier materials in depressions or gentle slopes, just as occurs in miniature in a miner's pan or rocker; and the result may be brought about by local as well as general glaciation, as is well shown in the valley of the Arkansas River at Twin Lakes.

"It is a very important fact that no pre-glacial caverns have ever been discovered" (p. 170)—a fact which leads to the inference that the extent of glacial erosion was so great as to totally remove pre-existing cavern-bearing limestone strata. The excavation of fiords and lake-basins, already adverted to, is a farther illustration of the enormous extent of this erosion. A most interesting, though indirect, result of this property of glacier ice is the influence which it has exercised on the social condition of mankind; for it is only shores indented by bays, fiords, and inlets, and fringed with islands, that afford the incentives to and facilities for the development of the maritime industries which occupy so important a place in human progress. The far-seeing geologist cannot, however, avoid speculating on the possibility that a contrary effect may, in the distant future, be exerted on mankind, by the return of glacial conditions to our globe. There is every probability, indeed, that the earth will again be enfolded in an icy mouth such as crept over it during the Quaternary; though there is no reason to fear that such an untoward vicissitude is imminent.

A glossary of some fifty terms, a bibliography of nearly seven hundred entries, and a four-page index with the plates and descriptions follow.

It has been the aim to present, in the foregoing paragraphs, a full synoptical *resumé* of the work considered, without approval or comment. Several passages which are regarded as either erroneous or misleading, or open to serious objection on well-established theoretical grounds, have, however, been quoted in the words of the author. A portion of these may be noticed in their order; reference being made to the pages on which they occur.

P. 28.—Geikie[1] regards ice-bergs as the terminal portions of glaciers, broken off by their buoyancy on entering the sea; Tyndall,[2] however, supposes that the masses break downward by their own weight; while Schwatka[3] has shown that they are formed in either mode according to the temperature of the sea—and, he might have added, other circumstances.

P. 31.—"From information derived from all sources up to the present time, it may be gathered that the unpenetrated area of about 4,700,000 square miles surrounding the South Pole is by no means certainly a continuous 'Antarctic Continent,' but that it consists much more probably partly of comparatively low continental land, and partly of a congeries of continental (not oceanic) islands, bridged between and combined, and covered to a depth of about 1,400 feet, by a continuous ice-cap; with here and there somewhat elevated continental chains, such as the groups of land between 55° and 95° W., including Peter the Great Island and Alexander Land, discovered by Billingshausen in 1821, Graham Land and Adelaide Island, discovered by Biscoe in 1832, and Louis Philippe Land by D'Urville in 1838, and at least one majestic modern volcanic range discovered by Ross in 1841 and 1842, stretching from Balleny Island to a latitude of 78° S., and rising to a height of 15,000 feet."[4]

Pp. 38, 98.—Geologists generally do not consider that the correctness of the views of Ramsey and others as to the repeated recurrence of glacial periods throughout ge-

[1] "Great Ice Age," Am. ed., 1877, p. 55.

[2] "Forms of Water," 1877, p. 134.

[3] *Science*, vol. ii, No. 30, 1881, p. 31.

[4] Sir Wyville Thomson, in addresses before the Geographical Section of the British Association, Dublin meeting. Brit. Assoc. Rep., 1878, p. 619; *Nature*, Aug. 22, 1878; *Am. Jour. Sci.*, vol. xvi., 1878, pp. 355-6.

ological time, is fully established; and they certainly do not regard the covering by ice of "a very large part of land and sea" as a "fact," or indeed as more than an extremely vague hypothesis.

P. 40.—Croll long since suggested[5] that Caithness and the Orkneys were glaciated by the Scandinavian ice-sheet, and Peach and Horne have recently urged[6] that during a part of the glacial period the Shetlands were overspread by the Scandinavian *mer de glace*, while during another part they gave origin to a system of local glaciers, though Milne-Home[7] seriously questions these several conclusions; and it has also been suggested by Reid,[8] though it can hardly be regarded as established, that the contortion of the drift along the Norwich coast was effected by Scandinavian land-ice. As shown by Croll's[9] and Geikie's,[10] as well as all other reliable maps, however, all the British highlands were unquestionably independent centers of glaciation, quite distinct from the Scandinavian ice-sheet;—indeed, when Milne-Home characterized the statement that "the land-ice which glaciated Scotland could only have come from Scandinavia" as an "astounding declaration,"[11] Peach and Horne hastened to explain that the word *Scotland* was a mis-print for *Shetland*.[12] There are also grave reasons to question whether a polar ice-cap ever existed in the northern hemisphere, as the writer has endeavored to prove,[13] and it is quite certain that if such an ice-cap did exist it did not extend to the British Isles by way of Scandinavia. So long ago as 1845 Murchison showed by means of a map in his "Geology of Russia and the Ural Mountains," that the Scandinavian drift "proceeded eccentrically from a common centre;"[14] and Geikie (illustrating his remarks by a map) says: "the direction of the glaciation in the extreme north of Scandinavia, the peninsula of Kola, and north-eastern Finland, demonstrates that the great *mer de glace* radiated outwards from the high grounds of Norway and Sweden, flowing north and northeast into the Arctic Ocean and east into the White Sea, and thus clearly proving [proves?] that northern Europe was not overflowed by a vast ice-cap creeping outwards from the North Pole, as some geologists have supposed."[15]

Pp. 41,44.—It is not known that a continuous ice-front ever stretched across the American continent, since a portion of the region eastward of the Cordilleras remains unexplored. Dana remarks, "since evidence of the great southward moving glacier fail over the region west of a line passing from a few degrees west of Winnipeg, southeastward through Western Minnesota and Iowa, near the meridians of 98°–100°, and all the way westward to the borders of California and Oregon if not to the Pacific coast, the ice thinned out toward the interior of the continent and was mostly absent except about the higher parts of the Rocky Mountains."[16] There is moreover no sufficient reason for believing that the American ice-sheet swept down from polar regions. Hitchcock's map of directions of ice-flow, which is reproduced in the work under consideration (following pl. XX.), indicates that the principal American center of dispersion was probably in the northern Laurentian Highlands;—a view which is corroborated by more recent observations of the Canadian Geological Survey.[17] Houghton has also shown that the boulders of the Arctic archipelago were carried northward.[18]

P. 42.—The quoted remarks describing the southern limit of ice-action in the Mississippi valley display unpardonable disregard of the work of Cox and Collett in Indiana, of Worthen and his associates in Illinois, of the several Missouri geologists, of White and St. John in Iowa, of Aughey in Nebraska, of N. H. Winchell and Upham in Minnesota, and even of Hitchcock's map appearing in the volume. The southern limit of the drift does not pass through either Iowa or Minnesota, but through southern Indiana and Illinois, northern-central Missouri, and Nebraska. The statement possibly originated in confounding the great Kettle Moraine with the southern drift-line, though as specifically pointed out by Upham,[19] and hardly less distinctly by Chamberlin,[20] these lines are 300 miles apart.

The glacial phenomena of south-western British America have been carefully and tolerably fully studied by G. M. Dawson.[21]

Pp. 66, 68.—An explanation of the origin of kames and aasor which is satisfactory to students of glacial phenomena was independently offered by N. H. Winchell,[22] Holst,[23] and Upham.[24] The hypothesis is as definite and probable as almost any other portion of the glacial theory.

Pp. 70, 90.—Tyndall has shown[25] that a diminution of terrestrial temperature could never inaugurate a glacial epoch; it has never been demonstrated that augmentation of temperature would be in any degree likely to produce a similar effect; and, accordingly, Poisson's and Le Coq's hypotheses can hardly be regarded as "reasonable" or "likely" in the present state of knowledge. As Croll has urged[26], there are grave reasons for questioning whether anything like half of the heat reaching the earth comes from the stars.

P. 75.—Loomis, describing the orbital motion of planets undisturbed by exterior forces, says: "the curve cannot be a circle unless the body be projected in a [particular] direction * * *, and, moreover, unless the velocity * * * is neither greater or less than one particular velocity;"[27] and Stockwell, speaking of the solar system in its actual condition, says: "the eccentricity of the earth's orbit will always be included within the limits of 0 and 0.0693888."[28]

P. 95.—Sound bases for the assertion do not sppear. Hilgard says[29]: "the relations mentioned by Tuomey (Second Report on the Geology of Alabama, p. 146) as existing between the shore of the Tertiary sea and the region of occurrence of the southern [northern?] drift on the Atlantic slope, are not so clearly recognizable in Mississippi and Alabama;" and further westward no relation whatever is apparent. Furthermore the "northern drift" of Hilgard and Tuomey was deposited far beyond the southern limit of the ice.

P. 107.—Le Conte shows graphically[30] that, admitting the influence of eccentricity, glaciation would only be possible after protracted secular refrigeration. The period in which this refrigeration reached such a degree as to permit of glaciation has not been determined. If in the Cambrian, Le Conte's diagram is, of course, incorrect; but it is not seriously maintained by working geologists in America that such was the case. Speaking of the Quaternary Le Conte, referring to Nordenskjold's observations,[31] declares[32]: "that glacial conditions were

[5] *Geol. Mag.* May and June, 1870; "Climate and Time," chap. XXVII.

[6] *Quar. Journ. Geol. Soc.*, vol. XXXV, p. 778; *Geol. Mag.*, II., vol. VIII, p. 65; *ibid*, p. 364.

[7] *Trans. Edin. Geol. Soc.* vol. III. pt. 3, p. 357; *Geol. Mag.*, II., vol. VIII., p. 205; *ibid*, p. 449.

[8] *Geol. Mag.*, II, vol. VII, p.——(A number of the writer's volumes are in the bindery at this writing, and a few references accordingly cannot be given in full.)

[9] "Climate and Time," p. 449.

[10] Accompanying "Great Ice Age."

[11] *Q. J. G. S.*, XXXV, p. 809.

[12] *Geol. Mag.*, VIII, p. 69.

[13] *Proc. A A. A. S.*, vol. XXIX, p——.

[14] Cited by Lyell, "*Antiquity of Man*," 4th revised ed., 1873, p. 275, note.

[15] "Great Ice Age," p. 354.

[16] "Manual of Geology," 3d. ed. 1880, p. 537.

[17] Geol. Surv. Con. Rep. Prog. for———, p——; *ibid.* tor———, p——.

[18] McClintock's "In the Arctic Seas," author's ed., appendix, p. 368.

[19] 8th Ann. Rep. Geol. and Nat. Hist. Surv. Minn., p. 73.

[20] Geol. Wis., vol. ii., 1877, pp. 205-15; *Trans. Wis. Acad.* of Arts, Sciences, and Letters, vol. —, p. —.

[21] *Quar. Jour. Geol. Soc.*, vol. xxxiv., pt. 1; Geol. Surv. Con., Rep. Prog. for—, p. —; *ibid.* for —, p. —.

[22] *Proc. A. A. A. S.*, vol. —, p. —.

ever before reached, even in polar regions, seems more than doubtful."

The foundation for the second statement quoted does not appear.

P. 108.—It has not been empirically established that the effect of fog-banks is to diminish temperature; and analogy with the known observation of finely disseminated water suggests that a directly opposite result ought to be produced. Croll's argument has been considered by Newcomb [33] and the writer.[34]

P. 113.—G. M. Dawson[35] has investigated the late-Quaternary depression of Vancouver's Island and British Columbia, and finds it to have been practically commensurate with that of the Atlantic coast.

P. 119.—Geologists are not united as to the age of the great pachyderms. Thus, Collett[36] records observations indicating that they were recent; while Phillips[37] and Godwin-Austen[38] regarded them as wholly pre-glacial, and Hall[39] and Belt[40] have shown that at least some individuals existed before the advent of the ice.

PP. 119-20.—Howorth has recently (mainly since the publication of "Glaciers") examined the evidence relating to the former existence of the mammoth in Siberia, and reaches the conclusion (among others). 1st, that the animals lived where their remains now lie; and 2nd, that the climate was comparatively mild at that period[41].

P. 134 A subsequent and apparently complete study of the locality by Lewis leads to the conclusions; 1st, that the implements are confined to the Trenton River gravel: and 2nd, that this gravel was deposited "at a period immediately following the last glacial epoch." [42]. If, as suggested by F. W. Putnam, [43] the flints were dropped into this gravel while in process of formation by the paleolithic men who hunted and fished along the old river-bluffs of New Jersey, it follows that these men were post-glacial; and even if the correctness of Lewis's views are not fully established, as intimated by Dana,[44] this instance does not demonstrate, or even indicate, man's pre-glacial existence.

P. 140.—Tyndall mentions[45] that Scheuchter first propounded the dilatation theory in 1705, that in 1760, or nearly forty years in advance of De Saussure, Altman and Grüner enunciated the sliding theory, and that in 1773, or thirty years before the publication of De Saussure's "Voyages dans les Alpes," the plastic theory was put forth by Bordier.

P. 142.—The re-opening, not incidentally or even judicially, but in a ludicrously partisan tone, of this now almost forgotten though erstwhile bitter controversy, would be quite unjustifiable even if the statements where not erroneous. Forbes' first visit to the alpine glaciers (as published by himself in that year), was on the 9th. of August, 1841; [46] during which visit he was in the company of Agassiz. Throughout this season his observations, as indicated by his published results, were confined to superficial phenomena; chiefly "ribboned structure" and "slaty cleavage." On the 24th of June, 1842, he again reached Montannent[47] with a set of instruments of precision, avowedly and obviously carried thither for the express purpose of instituting a series of measurements of the motion of the ice—the necessity for such measurements having been pointed out in lectures in December, 1841, and January, 1842,[48] and also in the *Edinburg Review* for April, 1842[49];—and the "First Letter on Glaciers," containing the "account of the first experiments, undertaken in June, 1842, to determine the laws of motion of the Mer de Glace of Chamouni" (published in October as already noted), was dated "4th July, 1842"; on which very day as stated by Dana on Tyndall's authority,[50] Agassiz' measurements proving the more rapid flow of the medial portion of the glacier, were published in the *Comptes Rendus.* Lyell, speaking of the more rapid medial than lateral motion of glaciers, says [51]; Mr. Agassiz, at p. 462 [of the "Systeme Glaciere"], states that he published in the *Deutche Vierteljahrschrift* for 1841, this result as to the central motion being greater than that of the sides, and was, therefore, the first to correct his own previous mistake." Comment is unnecessary.

Pp. 143-4.—Substances not previously regarded as viscous, as for instance Stockholm pitch "so hard as to be fragile throughout, and present angular fragments with a conchoidal fracture" and a glassy lustre[52], are also referred to by Forbes.

The "followers" who hold the view indicated are not advocates of the viscous theory proper, which is essentially molecular. The motion of viscous, as of fluid bodies, may, however, be very imperfectly *illustrated* by the movement of a heap of independent spherical masses.

P. 145.—The re-statement of this view, which it is painful for admirers of Croll's important labors in other directions to discuss, is hardly excusable. Readers may satisfy themselves as to its validity by referring to the criticisms of Blakie[53] and Teal[54]. The authors should have pointed out the differences between solid, liquid, and gaseous molecules of H_2O.

P. 148.—The motion of the alternately freezing and thawing snow unquestionably occurred in the manner assumed in the dilatation theory as advocated by Scheuchzer, Charpeutier, and, especially Mosely; but it is just as unquestionably distinct from the true flow of glacier ice. The recognition of miniature glaciers in the New England snows, far transcends the peculiar ideas of Muir, which are so strongly deprecated by King[55].

Pp. 151, 154, 159.—The extraordinary conclusions reached are perhaps to be attributed to the inadequacy of the theory of glacier motion adopted. The statements may be looked upon as representing unduly emphasized ideas of a purely speculative nature.

P. 165.—So long as a majority of leading students of Quaternary phenomena classify the upper and generally yellowish portion and the lower and generally bluish portion of the drift respectively as Upper Till and Lower Till, and look upon them as distinct in either time or mode of formation, as do Newberry, Upham, Hitchcock, Aughey, N. H. Winchell, Stone, and many others in the United States, and so long as these deposits are distinctly separated by a characteristic vegetal stratum, as they are at least in southern Ohio,[56] northeastern Iowa,[57] and Nebraska,[58] the first quoted statement must be regarded as unsupported by facts, notwithstanding the possibility that atmospheric and vegetal action might, as urged by Hawes,[59] Julien,[60] and Van den Bröck,[61] produce a similar discoloration of a single homogeneous formation having the constitution of the Lower Till; and since the bluish clays quite frequently (and indeed over some considerable

[23] *Geol. Fören. Stockholm Förh.*, Bd. iii.,No. 3, pp. 97-112.

[24] *Am. Jour. Sci.*, Lec., 1877; *Proc. A. A. A. S.*, vol. xxv., *p.* 216, *et req.;* and vol. iii. of the late New Hampshire reports.

[25] Heat as a Mode of Motion," Am. ed., p. 176; "Forms of Water,' p. 154.

[26] "Climate and Time," p. 39. Newcomb says (*Am. Journ, Sci.*, Vol. XI.,1876, p. 263)—"Practically there is but one source from which the surface of the earth receives heat, the sun, since the quantity received from all other sources is quite insignificant in comparison."

[27] "Treatise on Astronomy," 1876, p. 138.

[28] "Secular Variations of the Orbits of the Eight Principal Planets." *Smithsonian Contributions*, No. 232, 1872, p. XI.

[29] Geol. and Agricult. Miss., 1860, p. 28.

[30] "Elements of Geolo " 1879, p. 550.

[31] *Geol. Mag*,, Nov., 1875, p. 525.

[32] "Elements," p. 549.

33 *Am. Jour. Sci.*, vol. xi, 1876, p. 272.

34 *Popular Science Monthly*, vol. xvi, 1880, p. 816.

[35] *Vide*, note 21.

[36] Ind. Rep. of the Bureau of Statistics and Geol., 1880, pp. 384-6.

[37] "Geology of Yorkshire," 1829, vol. I, pp. 18, 52. cited by Belt, *infra*.

[38] *Reports British Assn.*, 1863, p. 68.

[39] 21st. Regent's Rep. on N. Y. State Cabinet, 1871, p. 103, *et req.*

[40] *Popular Science Monthly*, vcl. XII, 1878, p. 62.

[41] In an unfinished seri of papers in vols. VII. and VIII, of the *Geol. Mag.*

areas generally) approach or even reach the surface toward the southern limit of ice-action, as in southern Indiana,[62] Illinois,[63] Iowa,[64] and Nebraska,[65] and in northern Missouri,[66] the second statement must be viewed in a similar light.

P. 170.— The cave-fauna, especially in Europe, is essentially identical with that commonly regarded as Quaternary or Champlain ; and as already intimated, there is good reason to suspect that this fauna was at least partially pre-glacial. Moreover, the same facts relied on by the elder Buckland to establish the anti-diluvian age of the ossiferous cave-deposits,[67] must today be considered equally conclusive of the pre-glacial existence of these remains.

On passing to a more general survey of the work there is found to be less occasion for criticism, and indeed some grounds for high commendation. Thus, the influence of aqueous vapor and other substances diffused in the atmosphere upon terrestrial temperature is rarely lost sight of, the existing glaciers are faithfully and accurately described, and the general character and effects of Quaternary glaciation are fully and clearly dealt with. The teachings as regards the recurrence of glacial epochs and their influence upon the successive geological formations of the globe are, however intensely radical. The evidence relied on to demonstrate the glacial origin of sandstones and conglomerates, azoic shales and slates, and unoxidized argillaceous rocks generally, is quite inadequate. For instance, the idea that the iron of glacial mud alone is normally unoxidized while that of river silts is normally oxidized, appears to be expressly contradicted by the facts that the iron of the Lörs (which is almost certainly formed of impalpable glacial debris), is nearly universally peroxide, even when the deposit is one or two hundred feet thick, while that of the post Lörs alluvium of the lower Mississippi is invariably protoxide, even within a few feet or even inches from the surface; and the opinion that paleozoic ice-action cannot be proven by the same evidence as that attesting Quaternary glaciation is directly opposed by the facts that the Talchir (lowest paleozoic) beds of Peninsular India contain striated and polished boulders imbedded in the finest silt, and that the underlying Vindhyau (azoic) rocks bear similar markings ; though even in this case the original observers do not refer the phenomena, with any degree of certainty, to glacial action. Again, while the discussion of glacial hypotheses and theories of glacial movement is tolerably full and (unless possibly in a single instance) eminently impartial and candid, the " composite theories " adopted are quite valueless, since the detailed investigation given to the subjects is in almost every case much less searching and exhaustive than that upon which each hypothesis was originally based. It may be questioned, indeed, whether the method of throwing together a number of essentially distinct and imperfectly weighed hypotheses, and taking the sum or the mean of all as the only consistent theory, will ever come into general repute, however strongly it may be supported by a confusing array of glittering generalities. Thus, with regard to the problem of ice-motion; it is of course true that the dilatation, fracture-and-vegetation, and pressure-melting hypotheses are based on the observed behavior of ice ; but it does not necessarily follow that these properties, either individually or collectively, produce the phenomenon of flowing in large bodies of ice. To illustrate :—solids generally expand and contract with alterations in temperature ; they may be fractured by irregular strain or impact ; they may be united into homogeneous bodies by pressure as shown by Spring ; and those which expand on solidifying may be melted by pressure ; yet no physicist attributes the flow of solids (which has been investigated by Tresca, Roberts, Ware, and others) to any or all of these properties. Finally, there are important omissions, notably with respect to the widespread hipartite structure of the drift in many if not most glaciated regions, which has led many European and several American geologists to conclude that it was formed during two distinct periods, separated by a considerable era of mild climate. The general neglect of the results of American (and indeed Foreign) study has been incidentally noted in preceding paragraphs.

The illustrations, which are of the finest character and elaborately described, are mainly reproduced from photographs of existing glaciers and of interesting phases of glacial phenomena in America and elsewhere. They are of course open to the objection which may be urged against all photographs of natural scenery—*i. e.*, that the most instructive and valuable details are often obscured or concealed ;—though many of the plates are of remarkable clearness and beauty. The illustrations are not, however, superior or even quite equal to many which have already been published by Agassiz (for instance, in the atlas accompanying "*Etudes sur les Glaciers* ;" Neuchatel, 1840—" Untersuchungen über die Gletcher," Solothurn, 1841), and others.

It should be added that the necessity for all the foregoing criticisms appears to have arisen from the peculiar design of the work and the circumstances under which it was prepared. The authors explain in the preface that in order to meet the agreement with the publishers, it was necessary to prepare the text with far greater haste than was desirable; and remark that,—" if the reader will consider that the main object in the book is not to afford a complete history of glaciation, but to present a body of graphic illustrations of glacial phenomena, and that the text is designedly subordinate to this purpose, he will then better understand the apparent short-comings of the work."

Viewed as a whole, it appears that the work describes no new phenomena and presents no new theoretical views, while it exhibits many deficiencies and inaccuracies. It cannot therefore be regarded as in any sense a valuable contribution to the subject dealt with, or even as a satisfactory exposition of the present state of that subject. To the working student it will accordingly be worse than useless, since it will impose upon him a heavy financial and

[42] " Primitive Industry," Abbott, 1881, pp. 541, 551.

[43] 14th Ann. Rep., Peabody Museum, 1881, p. 23.

[44] *Am. Jour. Sci.*, vol. XXII, 1881, p. 402.

[45] " Forms of Water," pp. 155-7.

[46] *Edin. New Phil. Journ.*, Jan. 1842 ; " Occasional Papers on the Theory of Glaciers," 1859, p. 3.

[47] *Edin. N. P. J.*, Oct., 1842 ; Occ. Papers, p. 9.

[48] Occ. Papers, p. 10.

[49] " The solution of this important problem [the theory of glacier motion], would be obtained by the correct measurement, at successive periods, of the spaces between points marked on insulated boulders on the glacier ; or between the heads of pegs of considerable length, stuck into the matter of the ice, and by the determination of their annual progress." *Op. cit.*, p. 77 ; Occ. Papers, p. 10.

[50] " Manual of Geology," 1880, p. 694.

[51] " Principles of Geology," revised ed., 1854, p. 224, note.

[52] Occ. Papers, pp. 93, 269.

[53] *Geol. Mag.*, vol. III, p. 493.

[54] " A Criticism of Dr. Croll's Molecular Theory of Glacier Motion," London, 1880.

[55] " Systematic Geology " of the Fortieth Parallel Survey, 1878, pp. 447-8.

[56] Geol. Surv. O., vol. III, pt. I, 1878, p. 38, *et passim*.

[57] *Am. Jour. Sci.*, vol. XV, 1878, p. 339 ; *Proc. A. A. A. S.*, vol. XXVII, 1878, p. 198 ; *Geol. Mag.*, vol. VI, 1879, pp. 353, 412.

[58] Phys. Geol. and Geog. of Neb., 1880, p. 259.

[59] Geol. N. H., vol. III, 1878, p.333.

[60] *Proc. A. A. A. S.*, vol. XXVIII, 1879, p. 350.

[61] *Mem. Cour. et Mem. des Sar. Entr.* of the *Acad. Roy. de Belgique*, vol. XLIV, 1881. Noticed in *Am. Jour. Sci.*, vol. XXII, 1881, p. 80.

[62] Geol. Surv. Ind., 1872, p. 404 ; 1875, p. 171 ; and elsewhere.

[63] Geo. Surv. Ills., vol. III, 1868, p. 190 ; IV, 1870, p. 194 and elsewhere.

[64] Geol. Iowa, 1870, vol. I, p. 327 ; II, p. 9 ; and elsewhere.

[65] Geol. Surv. Mo., 1855-71, p. 162 ; 1873-4. p. 245 ; and elsewhere.

[66] Phys., Geog. and Geol. Neb., 1880, p. 254.—In each of the first four States above mentioned, the occurrence of the clay and its stratigraphical position has been determined mainly by personal observation.

[67] " Reliquiæ Dilurianæ." 1824, pp. 48-51, 171-84.

a no less mental tax, without adequate recompense. To the teacher, however, for whom it is especially designed, it will doubtless prove quite acceptable as an auxillary to the more elementary text-books.

Several considerations appeared to demand a rather full examination from the "standpoint of the working-geologist of Glaciers." (1.) There is so urgent a demand for a standard work representing fully the present *status* of American Surface Geology (or Kaineontology, as the writer prefers to term that branch of Geology), that almost any book on the subject might be adopted as such without duly weighing its fitness for the position. (2.) In its ambitious style and assumptious *ensemble* the work under review is quite unlike the ordinary text-books. (3.) It is the initial volume of an extended and costly series of works which, from their titles and the fact that they carry with them the prestige of a leading university, might naturally be regarded as the highest American authorities on the subjects treated. (4.) It was not deemed just to working geologists to suggest that the book could well be dispensed with without at the same time furnishing, as fully as practicable, the means of forming an independent judgment.

FARLEY, Iowa, *Nov.* 12, 1881.

LIVING OBJECTS FOR THE MICROSCOPE.

Mr. A. D. Balen, of Plainfield, New Jersey, has undertaken to collect living organisms suitable for microscopical investigations, and forward them by mail to those interested in such studies.

This is a great convenience to those living in cities, or those who are unacquainted with the localities where collections of particular forms can be made.

Among the living objects which Mr. Balen has sent out to his correspondents may be mentioned—

POLYZOA.—Pectinatella, Plumatella and Fredericella.

INSECTS.—Larva of Dragon Fly and Dyticus (water tigers).

ENTOMOSTRACA.—Bosmina, Daphnella, Diaptomus and Sida.

WORMS.—Nais, Stylaria and Planaria.

ROTIFERS.—Lacinularia, Conachilus, Floscularia, Melicerta, Limnias and Noteus.

POLYPS.—Hydra, with the curious parasite Urceolaria pediculus.

BELL ANIMALCULES.—Vorticella, Carchesium and Epistylis, Stentor, Vaginicola and Cothurnia.

INFUSORIA.—Spirostomium, Euglena and Dinobryon.

RHIZOPODS.—Arcella, Actinophrys and Clathrulina.

SPONGE.—Spongilla.

PLANTS.—Utricularia, Vallisneria, Anacharis and Nitella, Volvox, Protococcus and Pediastrum.

DIATOMS.—Surirella, Gomphonema and Fragilaria.

DESMIDS.—Scenedesmus, Desmidium and Micrasterias.

We hope that microscopists will support Mr. Balen in this little enterprise, for it will prove of the greatest benefit to them. A specimen package will be sent for 30 cents.

THE giant forces which scientific discovery is putting in the hands of engineers bid fair to develope a particular form of the profession.—*Engineering News.*

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING DEC. 24, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

DECEMBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Wet Bulb.	Time.	Maxi'm In Sun
Sunday, 18..	30.137	30.264	12 p. m.	30.100	7 a. m.	41.3	37.3	50	1 p. m.	42	1 p. m.	33	7 a. m.	32	7 a. m.	99.
Monday, 19..	30.309	30.382	10 a. m.	30.264	9 a. m.	40.3	38.0	45	3 p. m.	41	3 p. m.	34	8 a. m.	34	8 a. m.	85.
Tuesday, 20..	30.159	30.288	9 a. m.	30.112	3 p. m.	42.6	39.0	47	4 p. m.	42	4 p. m.	38	4 a. m.	36	6 a. m.	65.
Wednesday, 21.	30.276	30.318	10 a. m.	30.152	9 a. m.	40.7	37.0	43	3 p. m.	39	9 a. m.	38	8 a. m.	36	8 a. m.	78.
Thursday, 22..	29.864	30.228	9 a. m.	29.516	12 p. m.	49.3	47.3	53	4 p. m.	51	4 p. m.	39	2 a. m.	38	2 a. m.	49.
Friday, 23..	29.405	29.774	12 p. m.	29.268	1 p. m.	42.6	42.0	55	12 m.	53	7 a. m.	25	12 p. m.	25	12 p. m.	100.
Saturday, 24..	30.188	30.300	11 p. m.	29.774	9 a. m.	27.0	26.0	32	3 p. m.	31	3 p. m.	21	8 a. m.	21	8 a. m.	79.

Mean for the week .... 30.048 inches.
Maximum for the week at 10 a. m., Dec. 19th .... 30.382 "
Minimum " at 12 p. m., Dec. 23d .... 29.268 "
Range .... 1.114 "

	Dry.		Wet.
Mean for the week	40.5 degrees	....	38.1 degrees.
Maximum for the week, at 12 m., 23d	55. "	at 7 am 23d,	53. "
Minimum " " 8 am., 24th	21. "	at 8 am 24th,	21. "
Range " "	34. "	....	32.

DECEMBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND *SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration h. m	Amount of water	OZONE. 0 / 10
Sunday, 18.	w. s. w.	w. n. w.	n. w.	230	2	7.00 am	.168	.153	.199	89	44	74	3 cir. cu.	1 cir.	0	-----	-----	----	--	0
Monday, 19.	n. w.	s.	s. w.	92	1	11.10 pm	.183	.195	.221	90	67	83	1 cir.	1 cir.	0	-----	-----	-----	--	0
Tuesday, 20.	w. s. w.	w. s. w.	w. n. w.	182	2¼	3.15 pm	.194	.169	.208	81	54	75	8 cir. cu.	10	8 cu.	-----	-----	-----	--	0
Wednesday, 21.	n. n. e.	e.	n. e.	119	2	5.50 am	.173	.164	.181	72	58	73	2 cir.	9 cir.	10	10 pm	12 pm	2.00	.01	0
Thursday, 22.	e.	s. s. e.	s.	154	3	1.15 am	.231	.334	.348	83	86	86	10	10	10	9 am 7.45 pm	3 pm 12 pm	15.00 4.15	.23 .30	0
Friday, 23.	w. s. w.	n. n. e.	n.	265	12¼	5.00 pm	.376	.267	.174	87	100	100	10	9 cu.	8 cu.	9 am 5 pm	4 am 8 pm	4.00 3.00	.20 .01	10
Saturday, 24.	n. n. e.	n. w.	n. w.	178	2	1.20 am	.113	.113	.167	100	67	100	0	0	0	-----	-----	-----	--	0

Distance traveled during the week .... 1,220 miles.
Maximum force .... 12¼ lbs.

Total amount of water for the week .... .75 inch.
Duration of rain .... 1 day, 4 hours, 15 minutes

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.